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**TRACHEOTOMY WITHOUT TUBES.**

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*April 18. 77*

BY  
**HENRY A. MARTIN, M.D.,**  
BOSTON, MASSACHUSETTS.

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EXTRACTED FROM THE  
TRANSACTIONS OF THE AMERICAN MEDICAL ASSOCIATION.



PHILADELPHIA:  
COLLINS, PRINTER, 705 JAYNE STREET.

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## TRACHEOTOMY WITHOUT TUBES.

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IN performing tracheotomy for membranous croup, during the last sixteen years, I have dispensed entirely with the *canulæ*, or tubes, so generally considered indispensable. The number of these cases is but five. Two were successful; the patients are still living and well. In all, death was imminent, its postponement but a question of minutes. In one, permission to operate was given only when respiration had ceased, and the trachea was not opened till full three minutes after this cessation. In one case, which should not be counted in estimating the mortality, the operation was only performed to relieve the agony and prolong the life of the patient, an adult, both which ends were most perfectly attained. In only two of these cases was there anything like a reasonable possibility of success, and both recovered. I regret, of course, that I have so few cases to report, nor do I claim or assume in any way, that these two recoveries prove that tubeless tracheotomy is an advance in surgery. If all my cases, however, had died I should make that claim. The great advantages, seen at every moment and step of the treatment, subsequent to these operations, have convinced me and all the very numerous physicians to whom the cases were exhibited, of the great value of the method I advocate. That conviction would not be impaired if the accident of death had complicated them instead of recovery.<sup>1</sup>

I have not attempted to ascertain the various inventions and proposals which have been made with a view to avoiding and

<sup>1</sup> In neither of the five cases alluded to in this paper was croupal exudation at all perceptible in the fauces. In *all* of them it was, however, found in abundance below that point after tracheotomy. A sufficient commentary on the dogmatic teaching (which originated I think in Boston) that no case can be diagnosticated *membranous croup* unless exudation is visible on the *fauces*. In diphtheritic cases exudation doubtless generally, if not always, first appears in the *fauces*, but in even these it has often disappeared before the surgeon is called. In true croup I am satisfied that the exudation is *generally* confined to the air passages proper.

remedying certain acknowledged defects in the operation and apparatus of tracheotomy. There have been many such. It is quite possible, in the vast and fertile, if weedy, fields of periodical medical literature, that some method similar if not exactly like my own, may be recorded. If so, I am totally unaware of it, and in such a case should gladly waive my present claim to originality, for my object in presenting this paper is certainly far more to do a possible good to humanity, to add a line or two to the useful, practical lore of my profession, than to win for myself a modicum of renown as a surgical inventor or discoverer.

In the summer of 1859, I first performed tracheotomy in croup. The patient, a girl of five years, had the supreme misfortune to have a "skilful" mother who had dosed the child, for five days, with steadily increasing doses of wine of ipecac. It was kept in a continued state of *emesis*, without the slightest prophylaxis or arrest of the croupal exudation. I found the child, in the afternoon of the sixth day, almost in *extremis*. I recommended and urged immediate *tracheotomy*, which was refused. At midnight it was still persistently declined. The child was dying; it ceased to breathe, was dead to all intents and purposes, without the aid of surgery, and then consent was given. Ether was, of course, not used, for *anæsthesia* was as perfect as if the patient was not only *de jure*, but *de facto*, dead. The trachea was opened, the tubes introduced, and in half an hour the patient was breathing as tranquilly, and almost as slowly, as if in health. She survived thirty-eight hours, almost wearing out my dear friend, the late Dr. Nathan Hayward, and myself in the constant care and unintermitting watchfulness we both, turn by turn, gave her. My attendance on this case gave me clear practical insight into the vast and often insuperable difficulties surrounding the operation of croupal tracheotomy, and fully accounted for the lukewarmness with which many excellent and experienced men speak of it as a *dernier* and doubtful *ressort*, hardly worth consideration. In the latter part of the year 1859-60, a message was left at my house to visit a sick child about two miles away. When I arrived at the house I found a boy about two and a half years old *in articulo mortis* with croup. I told the mother that her child could live only a few minutes, and that I could do nothing. To her frantic inquiry if *nothing* could be done, if there was *no* chance at all, I told her, full of my recent experience, that there *was* an operation sometimes done in such cases, but it offered so little chance

that I could not recommend it, and, besides, I told her I had no instruments or tubes with me, and long before a messenger for them could half reach my house, her boy would be dead. While we were speaking, the child, gasping in its grandfather's arms, ceased abruptly to gasp, in fact, to breathe, just as had my first case. I had nothing like an instrument but a lancet which I used for vaccination. In my prescription book I had always two or three feet of silver suture wire, and a few surgical needles. I opened the trachea without an instant's avoidable delay, and, without moving the child from where it had ceased to breathe, after a few motions of Marshall Hall's method, respiration was re-established, large accumulations of mucus, and considerable shreds of membrane were expelled, breathing was soon easy, and the natural color of the face and general surface restored. I stitched the edge of the trachea (three rings of which had been divided) and the skin together on each side, and attached each stitch to a piece of an old elastic garter of the mother's carried round the back of the neck; little pads of soft rag were placed so as to prevent the wire—retracted by the elastic band—from dividing the skin and the tissues of the neck. I remained with the patient two or three hours, and, finding how easily the case could be attended, left it for several hours at a time through the whole course of the attendance. Observation of this case convinced me of the very great advantages of this operation and apparatus over any in which tubes were employed. The patient survived the operation for forty nine hours.

My second case, without tubes, was a girl of four, in whom death could certainly not have been postponed an hour without an operation. This case was successful. The silver sutures, with an elastic band, were employed, as also in the third, a most unfavorable case, complicated with pneumonia and bronchitis, which nevertheless survived over fifty hours, and would, I feel confident, have recovered, but for the brutal stupidity and idiotic folly of the mother, its only nurse. Various devices were contrived and tested, but abandoned, to replace the elastic band; but I will not trouble you with an account of them.<sup>1</sup> It was not till

<sup>1</sup> Many of these contrivances answered the purpose. They were complicated, and, being so, would probably attract the attention of those who have not learned the very important and positive fact that complicated contrivances in surgery are practically worthless. Everything in our profession that has endured, or been worthy to endure, to the fame of its author, has been simple, from Paré's ligature to Sayre's and Bryan's plaster jacket. It was not till I had reduced my

the fourth, a perfectly successful case, that I perfected the operation, or at least so nearly approached perfection, as to be willing to present it to the consideration and criticism of the profession. The operation, as I now perform it, will be perhaps best described and illustrated by a narrative of my fourth case. Before giving this narrative, I wish to allude to an important point observed in the use of the loops of silver suture wire attached to elastic bands. This was the marvellous rapidity with which the wires divided the tissues of the neck with all the clean directness of an incision, and quite without hemorrhage. This led me to using, with perfect success, a similar device in cases as, for instance, of *fistula in ano*, in which popular prejudice and weightier reasons made it desirable to procure bloodless division of tissues, and to avoid the use of the dreaded "knife." This, although analogous to the now extensively practised elastic ligature, is not the same, and was practised and reported by myself long before Professor Dittel's announcement of the method which has won him such well-deserved reputation. I hope, at some future time, to lay before this Association a description of these operations by means of loops of very delicate wire, constantly acted upon by elastic traction. I will only say now that, in the two very important points of much greater rapidity of action, and almost complete absence of pain, this method is far superior to that so commonly practised. With it the often very troublesome and painful tightening of the ligature by retying, necessary to effect complete division, is avoided.

On the morning of the 24th day of March, 1876, I was requested by Dr. J. S. Flint to visit with him, a boy aged seven, in the last extremity with croup, advise as to the expediency of tracheotomy, and if considered advisable, perform that operation.

The boy, eight days before, had worked for several hours clearing away snow from the sidewalk and paths about the house. From this exposure dated a "hoarse cold," which, as in so many thousand other instances, was not considered important. Domestic treatment was applied till the 23d, in the evening of which day Dr. Flint was called, diagnosed the case and its extreme gravity, and prognosticated a rapidly fatal termination without the aid of surgery. I found the patient with all the

operation to the utmost simplicity that it approached perfection. As done, at last, with simple thread sutures and properly adjusted strips of plaster, I consider it worthy to be offered to the profession, and of its serious notice and consideration.

often described signs of nearly impending death from croup. A thorough examination of the fauces revealed no appearance whatever of exudation; auscultation indicated almost entire extinction of respiratory sounds. Over the larynx, a fine, dry, almost whistling râle made evident the almost total closure of that passage to air. This sound was entirely free from a flapping or clicking character which has, in a few instances, been the only sign by which I have been able to diagnosticate membranous croup from oedema of the glottis. Notwithstanding the somewhat negative character of the symptoms, I agreed in Dr. Flint's diagnosis. The only question was as to oedema. This slight doubt was explained to the father, but he was also told, whether membranous croup or oedema, that the necessity for an operation to save life, however temporarily, was undoubted. The operation was urgently recommended, but with a perfectly candid and indeed exaggerated statement of the chances against success. The unfavorable points were that the boy was slender, nervous, and delicate; his mother had died of phthisis. In his favor was his age, the fact that, so far, both lungs were entirely free from either bronchitic or pneumonitic trouble, and that his surroundings were those of an intelligent American family in which decent and proper regard would be paid to a physician's wishes and commands. Consent was given. Ether was administered and, with the least possible delay, I opened the trachea as low as could safely be done, with the kind assistance of Drs. Flint, Streeter, Seavers, and S. C. Martin. The tracheal network of veins were very much engorged and tortuous, and although I succeeded in avoiding them, a profuse bleeding followed the knife and emptied the overloaded vessels of the skin. I waited for two or three minutes till this was accomplished, preferring very much to run the risk, which was imminent, of a cessation of respiration, rather than that of inhalation of blood, which has so often turned the scale at once against the success of tracheotomy. When the hemorrhage had entirely ceased and the wound was quite free from blood, I incised the trachea dividing three of its rings. The wound in the skin was about one inch and a half, that in the trachea not quite three-quarters of an inch in length. No exudation was visible through the opening in the trachea, the mucous membrane lining which could be seen, immediately after the incision, of a dark violet, almost black hue. In a few minutes this was very gradually changed to a shade but little

darker than the healthy normal tint. The sides of the wound were kept apart by small retractors, a large amount of retained mucosities was, as usual, in such cases, expelled, and, as soon as respiration had become comparatively tranquil, some ten or fifteen minutes, I introduced a single silk suture into each side of the wound. Each thread passed through the middle of the incised portion of the trachea at about one-eighth of an inch from its edge and through the skin at a corresponding point, and at about one-quarter of an inch from the line of incision.<sup>1</sup> The two edges, of the trachea and skin, were then approximated by tying them together, not so tightly as to at all incur the danger of "cutting-through" the tissues, but sufficiently to produce, when both sides were thus treated, a gaping of the tracheal wound and the establishment thus of an elliptical orifice. Two strips of good rubber plaster, which does not require heat to render it adhesive, were prepared, half an inch wide and about a foot long.<sup>2</sup>

<sup>1</sup> When, as is often the case, the trachea lies deep under engorged tissues, there is difficulty in readily passing the needle through an edge of the tracheal wound. In such cases, and indeed in all, the lip of the wound in the trachea should be caught by a small hook or *tenaculum*, and brought so near the surface as to enable the operator to pass the needle with ease. The small hook used for seizing and fixing the os uteri is exactly suited for this purpose; but a perfectly satisfactory substitute for it may be made by bending the pointed end of one sort of common silver probe, or of a piece of iron wire into a minute hook.

<sup>2</sup> What is called "rubber plaster" adheres well to the skin when it is perfectly fresh, but owing to its ingredients being dissolved and combined by means of a solution (naphtha or something of that sort), it becomes very soon practically useless and incapable of being made to adhere even by heating. I called the attention of the profession, some time since (in an article in the Boston Medical and Surgical Journal, October, 11th, 1877), to a plaster of my own invention, composed of rubber, burgundy pitch, etc., combined in the natural state, without any solvent, by being kneaded together by machinery, at a moderate temperature, which is probably quite free from this important defect of all previous surgeons' plaster, for now, two years after being made, it is precisely as adhesive as at first. This plaster is manufactured for Messrs. T. Metcalf & Co. of Boston, to whom I have transferred the commercial charge of the invention, and I hope it may prove, as it promises well to do, of real and important value to the surgeon. Ordinary sticking plaster (emp. adh.) is also worthless unless fresh, and, even then, can only be made properly adhesive (except in very warm weather) by artificial heat. It will be found, by sponging the surface of old sticking plaster, or even the old dry "rubber plaster" with chloroform or benzine, that it can be rendered extremely adhesive. This was a fact of great importance during our late war. We were able, in this way, to make use of the peculiarly worthless plaster (bad originally and made worse by age) supplied by the rascally medical contractors and storekeepers, to the surgeons of the United States Army.

A quarter of an inch from one end each of these was doubled on itself so that the adhesive surfaces came in contact. Through a puncture in the middle of this strengthened portion of each strip was passed one of the threads, still uncut beyond each knot, and this thread being tied to the other attached the loop through the skin and trachea *closely* to the end of the plaster strip. When both the pieces of plaster were thus attached, traction was made very gently on both sides (one after the other) to a degree to distend the wound and leave a fully sufficient aperture for the admission of air and the expulsion of secretions and of the *débris* of the disease, but carefully avoiding such a degree of traction as should at all endanger a cutting through by the thread of the edges of either the tracheal or cutaneous wounds. Each plaster strip was then laid smoothly and closely around the side and back of the neck, its end laying on the shoulder. The end aimed at is to make a sufficient opening to allow the admission of an ample supply of air and to steadily maintain that opening till the processes of nature shall remove the obstruction in the natural passages. To do this in such a way as shall offer no impediment whatever to sponging and wiping the anterior surface of neck as often and freely as desired, is a most important point. You will excuse me from giving a detailed account of the progress of this case to recovery; my object being, not to describe the entire treatment of croup, but only a modification of an operation which often forms, and much oftener should form, an essential part of that treatment. The correctness of our diagnosis was made manifest the day after the operation by the expulsion of two large strips of tough elastic croupal membrane, and on the third and fourth days of a large amount of the same substance in a disintegrated state. Some nine or ten days elapsed before air passed perceptibly through the natural outlet, and nearly twenty before it passed freely. These details, however, are common to all cases of recovery after tracheotomy in croup. It would be unprofitable to extend them. I will keep to my sole object in this paper, viz., to demonstrate that the operation without *canulæ* is infinitely to be preferred to the usual mode. What are its advantages? (1) It requires no apparatus. Thousands of patients have died without the only real chance which our art can offer being afforded them, for no other reason than that neither the attending physician or his neighbors had immediately at hand a set of the indispensable *canulæ*. (2) A much

larger and less impeded opening is maintained. (3) The continual and repeated irritation from the presence of the *camile* and their removal and re-introduction, to the lining membrane of the trachea, is entirely avoided. I have not the slightest doubt that even in skilful hands, this has sometimes turned the scale against the patient. (4) It is of the greatest importance that a free and unimpeded exit should be provided for mucosities and the *débris* of exudation. It is the continual and often instantaneous deposit of these matters which renders the constant sleepless attendance of not a mere nurse, however skilful, but of an exceptionally competent medical man absolutely necessary under the present method, to afford the best possible chance of recovery.

Besides what clogs up the inner tube, removed from time to time, the presence of the outer tube in the trachea leads to an accumulation below it, which, I feel confident, is one of the most frequent causes of death in croup after the operation. The secretion from the bronchi in these cases differs very much from that found in ordinary bronchitis. Although when first thrown out it is fluid enough to be readily expelled through a free opening, it becomes by exposure to the currents of inspired and expired air, of a viscosity resembling and equaling that of bird-lime, far beyond that of glue which it very much resembles in appearance. I believe it is the general opinion that croup proves fatal after tracheotomy, from the supervention of bronchial or pneumatic inflammation, but above all, by the extension and reproduction of membranous exudation. I have for more than thirty years given these cases most anxious study, and have never seen any reason to believe that after the operation of tracheotomy is performed the membranous formation advances or increases at all, or that it is in these circumstances ever reproduced on a surface from which it has spontaneously (*i.e.*, without violence) been separated.

A condition of intense congestion, evinced by a dark color of the faucial mucous membrane, an almost black hue of that lining the trachea is, I think, a necessary precedent and condition of the formation of a true croupal exudative membrane. This extreme congestion is relieved rapidly and almost entirely, as I have repeatedly seen, by the always copious hemorrhage following the incision through the congested skin and other tissues, and especially the mucous membrane. The extremely tenacious and albuminous secretion alluded to, resembles, doubtless,

that from which the croupal membrane is formed, differing from it in tending, not to the formation of membrane (so called), but of masses of an extremely tough adhesive character fitted, beyond comparison, to plug up bronchial passages, and keep them plugged, and so to induce gradual, progressive *asphyxia*, from the increasing debility of the patient preventing their expulsion. It is, in my opinion, the chief source of danger after tracheotomy. Its prevention, if possible, its ready removal when present, must always be most important ends to be attained.

No physician who has ever really and carefully watched a case of croup after tracheotomy will dispute what I have said of this peculiar substance, or that it gives infinite trouble, although he may not agree with me in attributing to it such a formidable and often fatal influence against the recovery of the patient. I will state how I direct the nurse to manage, and I feel now no hesitation whatever in leaving a case in the hands of an ordinary nurse, a thing I never dared or would dare to have done for ten minutes with the tubes. Instead of filling the room with vast volumes of steam to debilitate the patient and endanger the health of the attendants, I cut two or three good sized pieces of cheap, loose-textured Florida sponge into slices three-quarters of an inch thick. These are placed in a basin of *very* warm water, the temperature of which is reinforced and maintained by occasional additions of hot water. One of these slices or discs of sponge is squeezed out and laid over the wound in the neck. The open texture of the sponge permits the ready transit of air to be inspired, and in its passage it takes up vastly more warmth and moisture than is found in an equal volume of the atmosphere of a steam chamber, either in or out of hospital. In the act of expiration mucus and débris are expelled with force against the lower surface of the sponge, and become adherent thereto. Every ten or fifteen minutes, or at longer or shorter intervals, the sponge is removed, thrown into a second vessel of water, and replaced by another. When, after a short interval, a slice of sponge is removed, a very considerable amount of mucus, etc., will be found adherent and entangled in its hollows and foramina. I do not consider the constant application of these discs of sponge necessary, but their occasional and at first frequent use is found to be soothing and comfortable to the patient, and has some effect doubtless in promoting expectoration, and possibly some little advantage in preventing the inspissation of secretions.

in the more or less remote bronchi. Now and then, pieces of membrane, often of considerable size, will be thrown out of the wound with violence, and so suddenly withdrawn on inspiration as to elude the quickest fingers or forceps.

Any one who has watched a case well knows what trouble and distress are caused by these shreds of membrane, what great and imminent danger, if tubes are in the tracheal wound, and no prompt and skilful person at hand to remove them instantly. My directions, when a shred of membrane or a mass of tenacious mucus presents at the opening, are gently to pass a soft moist cloth or sponge back and forth across the wound. In a very few seconds the mass is flung against the cloth or sponge, and adheres sufficiently to it to be easily withdrawn. From time to time, in spite of these measures, the passage for the air becomes so much narrowed by a drying of mucus on the walls of the trachea and over the surface of the wound, that I am in the habit of syringing out the trachea.

The instrument I use for this purpose is a hard-rubber syringe with a silver tube bent on the quarter of a circle, at the end of which is a hollow sphere of silver of the size of a small pea, pierced with minute holes through which fluid may be forced in all directions. My custom is to fill this syringe with lukewarm water, to which common salt is added to render it as salt as sea-water. The bulb is introduced quickly through the wound, passed an inch or so downward and the salt water injected with some force. The effect of this is always to induce violent expirating paroxysms, the fluid thrown into the trachea tends to dissolve and dilute, part of the fluid runs down into the bronchi and is brought up again, bathing their sides, loosening and dissolving the layer of inspissated mucus. In a minute or two after the first injection, a second is thrown in, which is at once followed by such an expulsion of mucus, etc., as insures great relief to *dyspnoea*. It is instructive to notice how immediately temperature and physical signs of bronchitic and even pneumonitic trouble are modified and disappear after this second injection. There is not the slightest necessity for a physician or even a specially skilled person remaining with the patient to administer these injections. The bulb is introduced and withdrawn with great facility. In my fourth case, both father and aunt gave almost all the injections, and, finding how easy and innocuous they were, and what relief followed their use, even oftener than I had directed. I do not

know any objection to allowing the sutures and plaster straps to remain *in situ* till natural respiration is restored, but this is not absolutely necessary. In my first successful case they were removed on the third day, so fully had plastic exudation bound together all the tissues involved in the incision that the whole track remained open after they had been removed. In my fourth case I removed the stitches and plasters on the fifth or sixth day.

The physician who is called on to recommend tracheotomy in croup is placed in a most responsible and even painful position. If he does his duty and recommends the operation long before the desperate and almost hopeless state in which it is usually recommended and done he is liable to innumerable annoyances and *inter-professional* villainies. If, after such recommendation, not adopted, the patient recovers even under his own care, the case is forever spoken of as an evidence of his want of professional knowledge. If recovery takes place in the care of that ubiquitous relative, the "professional brother," he will never cease to be aware of the parallel which is drawn in his disfavor. Then again, no matter how strongly and truly you may tell the family the phenomena to be looked for after tracheotomy, the great, wonderful, and immediate relief, so often treacherous and gradually deepening into *asphyxia* and death, you cannot thereby avoid much chagrin and erroneous judgment.

I will not go on to enumerate all these *desagrémens*. They are very familiar to every man of any considerable experience who has done his duty or tried to do it.

It is the opinion of the ablest writers on the subject, one in which I most heartily coincide, that tracheotomy should be one of the *premiers* rather the very "*dernier ressort*" in all cases of croup of a clearly membranous character. I feel very confident that, if tracheotomy were understood to be, as performed as I advise, it is an operation of very little gravity, in no way increasing any chance against the patient, when done in the earliest stage of cases known or even *suspected* to be membranous, recovery would be the rule and not the too rare exception. As it is, it may be said that no case is operated on till it is practically in the very "*jaws of death*." Before justice is done to tracheotomy in croup, the people and the general profession have got to learn that it is not a difficult or dangerous operation; that, done soon enough, and the patient properly seen to afterwards, it offers not a faint, uncertain, and almost forlorn hope, but a decided, clear, and far from desperate prospect of entire relief and recovery.

If, however, not a single case of croup were saved from death by tracheotomy, a long experience and careful, anxious observation of several cases in my own practice and that of others, in which death has not been averted by the operation, convince me that with a view to that "*Euthanasia*," which Bacon has so wisely and truly said is a part of our art, a most important and neglected part,<sup>1</sup> I should still warmly advocate tracheotomy in croup. Where a patient dies of croup, unless the disease and exudation have been exceptionally gradual and slow, death is as distressing and dreadful, apparently to the subject, and most certainly to all surrounders as can well be imagined; a fearful gasping, writhing struggle and wrestling against inevitable fate. When tracheotomy is performed, even in the most desperate condition of such a case, the immediate effect is perfect relief and repose. Sooner or later, too often, this pleasant condition changes, but it does so very, *very* gradually; less and less oxygen reaches the blood, more and more carbonic acid slowly accumulates in the circulation. The effect of this is a gradual *asphyxia*, a slow and agreeable narcotism. The patient suffers no pain, but often, very often, affords signs of satisfaction and comfort, and so, gradually, from one stage and degree of insensibility to another, breathes more and more shallowly, and at last dies so quietly, and peacefully, and painlessly, that I have been in doubts for even as much as a minute after actual death whether the patient were not still breathing.<sup>2</sup>

<sup>1</sup> "Nay, further, I esteem it the office of a physician, not only to restore health, but to mitigate pain and dolours; and not only when such mitigation may conduce to recovery, but when it may serve to make a fair and easy passage; for it is no small felicity which *Augustus Caesar* was wont to wish to himself, that same *Euthanasia*; and which was specially noted in the death of *Antoninus Pius*, whose death was after the fashion and semblance of a kindly and pleasant sleep. So it is written of *Epicurus*, that after his disease was judged desperate, he drowned his stomach and senses with a large draught and ingurgitation of wine! Whereupon the epigram was made *Hinc Stygias chius hauxit aquas* (He was not sober enough to taste any bitterness of the Stygian water). But the physicians, contrariwise, do make a kind of scruple and religion to stay with the patient after the disease is deplored; whereas, in my judgment, they ought both to acquire the skill and to give the attendances for the facilitating and assuaging of the pains and agonies of death." Bacon "Of the Proficiency and Advancement of Learning." I believe it is usual to quote this work as the "De Augmentis," etc., and in the Latin, but as Bacon wrote the beautiful old English, and the Latin translation was made by some of the garreteers of Grub Street, I forbear the cheap and easy, and *usual* display of erudition.

<sup>2</sup> In this paper I only allude to cases in my own immediate practice. Several

This is the EUTHANASIA, and by and by, when our art becomes generally a far higher one than it now is, and has quite ceased to

operations have been done by others in the way I recommend, but I have seen none of these, nor can I furnish any details of them here. All from whom I have heard assure me of their perfect satisfaction with the tubeless method. A fifth case to which I allude, but do not here narrate, was of great and peculiar interest; but the necessary limits of these papers, stretched to the utmost, would not admit a proper account of it. The case peculiarly illustrating what I have said of the Euthanasia, I will here briefly narrate it. A young, healthy, and handsome Irish woman, cursed with a drunken husband, caught, in common parlance, a "bad cold." She became extremely hoarse, so that, even on the second day, she could hardly speak intelligibly; but as she suffered no pain (with many the only test of danger) her case was neglected. On the morning of the seventh day respiration was so much embarrassed that her husband was sent to summon me. He, however, preferred to go on a spree, and it was not till late in the evening that a messenger reached me. I found the patient quite unable to utter any articulate sound above a very faint whisper, imperfectly audible when my ear was placed over her mouth. Respiration was extremely difficult. The patient writhed about in every possible way, as if in a vain endeavor by some change of position to get more air. The idea that at once suggested itself was of one struggling while the throat was fiercely grappled by an assailant. On examining the fauces no membranous exudation was visible. Auscultation revealed an almost entire extinction of respiratory sounds; over the larynx a distinct flapping, or rather clicking sound could be heard, as of an edge of membrane moved by the slender but quick current of air passing through it. On percussion, the whole chest emitted, not the resonant sound found when the obstruction is in the larynx and trachea alone, but universally a dullish sound, unlike that of effusion or the hepatization of pneumonia, much less flat than either of these. The diagnosis which I made, but should probably not have made were it not for having observed once before, in an adult, a precisely similar case, was not of tracheal croup alone but of capillary bronchitis, with exudation in the entire system of bronchial ramifications. The poor woman read in my face that I considered her case hopeless. I had operated successfully on a child *in extremis* with croup in the neighborhood a few months before, and she intimated her wish to have tracheotomy performed. I told her how different was her case from that of the neighbor's child, how utterly hopeless, but that I thought some relief might be gained by the operation from her terrible suffering. By the light of a single flaring kerosene lamp, and with the kind assistance of my friend, Dr. Joel Seavers, of Boston, I opened the trachea as low down as possible, dividing four of its rings and bringing into view a dense wall of membranous exudation; this was also opened by a free incision, but without the slightest relief. I seized hold of the membrane with forceps, but it was quite immovable on repeated efforts. The patient was apparently expiring. She made one terrible struggle for breath, seeming to summon every remaining particle of her strength and vitality to the effort; I saw that the membrane suddenly moved slightly; I passed my forefinger through the incision and seizing the membrane as firmly as possible between it and my thumb withdrew a cast which, in its fresh state, measured nearly fifteen inches in length, and which now, after many years' immersion in alcohol, measures about eleven inches. A cast, evidently of the entire system of bronchial

pander to that folly of the people, born of their ignorance and fear, which seeks to shun the inevitable by tormenting, often torturing, useless, abominable so-called "remedies" for cases beyond all saving remedy, this easy, agreeable death will be aimed at and striven for by physicians, and its attainment con-

ramifications of one lobe. Respiration and percussion revealed the fact that the wonderful specimen I have mentioned had occupied the upper lobe of the left lung. From the time of the operation air could not be heard to enter at all either the lower lobe of the left lung, nor any part of the right, but it entered the upper lobe of the left lung with great intensity, producing a peculiar sound of marked puerile respiration, but without the breezy character which indicates full, healthy breathing and penetration by the air of the entire system of pulmonary air-cells.

The ease of her breathing, when it was evident that but about one-fifth of her lung tissue was capable of its function, and that imperfectly, was very remarkable and instructive, and I would say here that I have had several other opportunities of making this observation, viz., how small a portion of quite or nearly healthy lung will supply the blood with necessary oxygen. The patient survived for nearly seventy-eight hours, during a large part of which time I remained with her. During the whole of that time she was free from distress, although it was very evident that, very gradually, the system was receiving less and less oxygen. Respiration was, as it were, more and more shallow, and, of course, increasing in rapidity. Half an hour before death I was sitting by the bedside, eating, as a sort of lunch, some chocolate drops, I handed two or three to her; she received them, smiling. I asked her if she liked them; she nodded; if she was quite comfortable, happy, and free from pain and distress. She smiled brightly, and nodded three or four times. So she continued, quite conscious and comfortable, till a minute or two before death, which was perfectly quiet, so quiet that its exact period was inappreciable. Was not this the Euthanasia?

The specimen was exhibited to Prof. J. B. Jackson, and of course excited his admiration and desire to possess it for the college museum; but as that already had a similar and remarkable, though not one-half so extensive a cast of the sort, I presented it to the National Museum at Washington, where it may now be seen. One thing occurred in the course of this case which may as well be just mentioned here. About nine hours after the operation, I, very much fatigued, was lying on the bed by my patient, and had inadvertently dropped off to sleep. I was awakened by her violent movement. Just as I fully awoke the movement ceased and her breathing also. I passed a tube down to the bifurcation of the bronchi and inhaled with force, as I attributed the obstruction of breathing to a plug of croupal mucus, which proved to be the fact. Its dislodgment, however, was not followed immediately by return of respiration. I then began Marshall Hall's simple method of artificial respiration, by rolling the patient over on the side, then forward till the front of the thorax rested on the mattress. I had, before this, some doubt of the efficacy of this method, but never have had any since. At each motion the air could be heard rushing out and in, particularly the latter, with a clear, distinct, almost musical sound, easily heard in the adjoining room, and, in a few minutes, breathing was again restored, calm and tranquil.

sidered only second, as a professional triumph, to the direct and indubitable salvation of life.

All the sound arguments are in favor of early tracheotomy in croup. If I have proposed, as my experience leads me to believe, a method which renders that operation and its all important after care simpler, safer, infinitely more easy, more likely to accomplish what we desire, and entirely within the means of every physician worthy of the name, I have done a service to humanity and my profession which I doubt not will, by and by, be appreciated, if not at present.

JANUARY 1st, 1878.

[Since this paper was written, I have performed tracheotomy in three cases, all of diphtheria. In neither of these was the operation recommended, much less urged with any hope of recovery. In two it was done at the special wish of the patient's families and attending physicians, with a view to prolonging life and the euthanasia, both which ends were attained. In the third case tracheotomy was performed contrary to my recommendation, at the urgent request of the father. The patient, a fine boy of seven years, was attended by a homœopath, with whose fraudulent sect this suburb of Boston is much infested, thanks to the influence of the "Sage of Boston Highlands," whose numerous writings, worthless to the profession, have been widely circulated among the laity, and preaching as they all do, utter contempt and distrust of all medicine and medical practice and practitioners, except their author, have very naturally driven people to men who have, or at least profess to have, an abiding faith in their dilutions, triturations, and potencies. This homœopath, after six days' attendance, informed the father that the boy would die within an hour or two, that he generally succeeded in driving the disease upward into the nose, but in this instance it had, owing to his not having been called in soon enough, not been possible to prevent the disease from going, most perversely, in spite of all his faithful efforts, downwards into the trachea and lungs. The father came for me. I confirmed the diagnosis of my learned predecessor, and said that the boy could hardly live more than ten or twelve hours. The father asked me about tracheotomy, but I told him that I could not recommend it; the dyspnoea was not very distressing, and the prospect of success was, in my estimation, nothing. Just twelve hours afterwards, at midnight, I was again summoned to the boy—he was dying, and about ten minutes after reaching him, he ceased to breathe. The father insisted that I should operate, and I did so. After about eight minutes of Marshal Hall's method, automatic unconscious respiration began, and it was fully half an hour before there was the slightest consciousness. The boy lived fifty-seven hours.

Although all these cases were fatal, they all proved most fully the value of the method I advocate. The patients were all left in charge of their usual non-medical attendants from visit to visit, and in neither did any accident indicate that it was unsafe so to leave them. I wish here to protest against the very fashionable confusing of the two entirely distinct diseases, true membranous croup and diphtheria complicated with an obstruction of the air-passages due to congestion plus a certain exudation. I am quite well aware that gentlemen who get their knowledge of disease, not at the bedside, but in German Encyclopædie and

Etymological Dictionaries, make out a very plausible argument for the identity of the diseases, based principally on the Greek derivation of the word *Diphtheria*. I am also aware that some cases are, as it were, mixed; that cases of true croup may nearly approach true diphtheria, and vice versa; but no practitioner of large experience, and careful, just observation can doubt for a moment that the sthenic, perfectly non contagious disease, properly called croup, the membrane of which is simply superimposed on the epithelial surface, from which it easily separates to such a degree that a perfect cast of the entire system of bronchial ramification of an entire lobe may be removed with ease, a disease in which, no matter how long it continues, no fetor is observed, and in which when and while the obstruction in the air-passages is removed, the patient is well, strong, with good appetite and normal temperature is the same disease (not *similar* but the *same*) as the asthenic, intensely contagious affection the so-called membrane of which is invariably a necrosis of the mucous membrane plus, it is true generally, but by no means always a certain exudation, which is perfectly incapable of being removed except by such proceedings as would remove an equal portion of that mucous membrane, which essentially it is, till the process of sloughing detaches it. A disease in which the most horrible fetor is a constant concomitant, a disease in which when obstruction to respiration exists and is removed, and continues to be absent, the patient still goes on to die from the constitutional effect of the disease, quite apart from any disturbance of respiration or of the oxygenation of the blood. A disease in which even when recovery takes place it is very slow, gradual, and often imperfect, extending through months and even years of debility, and of a curious form of paralysis.

In a word, true inflammatory membranous croup kills by obstructing the passage of air alone. If the patients could somehow be furnished with air enough for a week or two they would all recover. Diphtheria, on the contrary, although in one of its complications, the immediate cause of death is obstruction to respiration, still kills a great many whose respiration is unimpeded, and very many, nearly all in whom, by operation, such obstruction is removed. Every physician however humble, has a right, nay it is his solemn duty to express his honest convictions, and I here express mine, that the modern theories by which croup and diphtheria are confounded, and the treatment of both founded on certain very theoretical notions of septic germs, etc. etc. etc., is a great misfortune and mistake, leading to a perfectly worthless mode of practice, and the abandonment of certain methods, particularly the proper use of calomel, in which, and which alone, immense experience had taught the profession that any confidence could be placed. The present fashionable *total* abandonment of the use of a remedy simply because the Thompsonians and other quacks raised a hue and cry against it and us, and also because its immense and thoroughly ascertained value in certain large and important classes of diseases led to its most improper use, on theoretical grounds, in diseases in which it does only harm, is very unfortunate and irrational. Men now boast that they never gave a dose of calomel. They make this boast among their patients, and often find the insinuation that a professional brother *does* use it, a most efficient means to win confidence in themselves, and shake the reputation of a rival. They only manifest their total unfitness to practise a great profession; that they are mere blind followers of the fashionable, evanescent teachings of the hour, and quite ignorant of the vast and neglected history and literature of our profession, which, whilst it records many errors which we do well to know and avoid, records, too, many of the precious results of experience, wisdom, and observation which, in the changes and chances of

medicine as taught by the fashionable professors of world-wide fame, are lost or forgotten. One of the great dangers of our time is the too hasty adoption of brilliant theories and of methods of practice based thereon; the too indiscriminate condemnation of any thing old because it is not new. Fashion changes in medicine as wildly and irrationally as in any thing else. I can remember the time when a very large and respectable State Medical Society listened with applause, certainly without derision, to an old practitioner who boasted (and made it the subject of his annual oration) that he had extensively practised obstetrics some forty odd years and had never used the forceps. He did not dwell on his cases of craniotomy, death from exhaustion, convulsions, vesico-vaginal fistula, the weeks, months, years, ages of torture suffered by the poor victims, his patients. That was not much more than twenty years ago. Would even that same society, however conservative, think such obstetric practice *very* glorious now? The fashion will change in regard to calomel, is even now changing, and a man who ostentatiously boasts that for a long life of practice, he has never used the lancet or given a dose of mercury, will be esteemed, and most justly, a fool, a "physician" who has all his life practised his profession without two of the three most potent and valuable means of the true art of healing. When the fashion for bleeding and calomel again returns, as it most surely will, let us hope that it will be tempered by wisdom, and not run into the absurdity and over-employment which fifty years since well nigh converted two of the most admirable remedies of the true art of healing into banes and curses to humanity.]

and the other, the place where the corresponding primary floor, and not the intermediate floors, are situated, and the distance between them is usually considerable. In such buildings, the intermediate floors are usually of considerably reduced height, so that the total height of the building is approximately equal to the height of the intermediate floor plus the height of the primary floor. This is the case in the majority of modern buildings, and it is also the case in the majority of old buildings, though the latter are more likely to have intermediate floors of equal height to the primary floor. The reason for this is that the intermediate floors are usually of smaller height than the primary floor, and therefore the total height of the building is less than the height of the primary floor.



